DISCUSSION ON THE ABOVE THREE PAPERS BEFORE THE RADIO SECTION, 11TH APRIL, 1951

Dr. R. L. Smith-Rose: The papers that have been presented are of the greatest interest, particularly in view of the trend of radio development into the very-high-frequency bands. That the subject is topical is clear from the reference made to it in the recently published Report of the Beveridge Committee, where some attention is given to the possibilities of what is termed "higher-frequency broadcasting." It is recommended in the Report that the broadcasting authorities should give the matter very close attention and should take active steps to explore the possibilities of extending broadcasting into the field of frequencies between 30 and 100 Mc/s—the range with which these papers are concerned. This recommendation is made particularly with a view to avoiding or overcoming some of the results of the congestion that is being experienced in European broadcasting in the medium and long wavebands and to providing the opportunity for a greater variety of programmes in the broadcasting service of this country.

Although the recommendation has appeared only within the last month or so, the paper by Messrs. Kirke, Rowden and Ross shows quite clearly that the B.B.C. have not been idle in this field, but that they have been very alert to the possibilities of development in a particular frequency band. The paper covers the phenomena of propagation from the transmitting station out to approximately the optical horizon, because it is within this area that a reliable broadcasting service can be obtained. The papers by Dr. Saxton and Messrs. Luscombe and Bazzard deal with the phenomena of propagation that take place at considerable distances beyond the horizon. This is of great

interest, not only for discovering the possibilities of longer-distance communication, but also for estimating what interfering field-strengths are likely to be received from broadcasting or other stations operating in the same frequency channel at much greater distances.

One of the features encountered in the study of wave propagation at frequencies of this sort is the variability of the results obtained. For short-distance transmissions, the field strength depends not only on the distance from the sending station, but also on the nature of the terrain between that point and the receiver. Over a smooth path it is possible, from a theoretical point of view, to establish a law relating the decrease of field strength to distance, but the theory gives only a general idea as to what is likely to happen when the path includes the various excrescences which are on the earth, as we find it, in the way of natural undulations, trees and the other usual obstructions. When the receiver is moved further away, another type of variability is encountered arising from the effect of the atmosphere on the transmission.

For such reasons, it is necessary to adopt a statistical approach in research in this field, and that is what the authors of all these papers have done. This point needs emphasizing as some investigators publish results of their detailed measurements which, because they are not interpreted in a statistical manner, fall short of providing as much information as would otherwise be obtainable in their publications.

I should like, if I may, to make one or two more detailed comments on the papers. During the reading of the paper by Messrs. Kirke, Rowden and Ross, attention was drawn to the differences in effect that are sometimes obtained in the use of horizontally- and vertically-polarized waves. In one of the contour maps, which illustrate the position very well, the manner in which horizontal polarization gives a stronger field so long as one is retreating from the transmitter up rising ground was shown particularly; but when one has passed the crest of the hill and gone over the top into the shadow, the vertical polarization recovers and gives a stronger field than the horizontal polarization. The contour map in Fig. 8 demonstrates this very well.

In this respect, I should like to draw the attention of the authors to a paper by Drs. McPetrie and Saxton,* which describes the results of some work in the propagation of waves on frequencies very close to those with which we are now concerned, namely 100 and 150 Mc/s. They brought out exactly the same point.

In a more critical vein, I would remark that the contour maps in Figs. 7 and 8 are not quite as clear as they might have been. I found Fig. 7, for instance, particularly difficult to understand. On the other hand, Fig. 21 is an admirable example of how a contour map should be presented. The dominating feature that strikes the eye at once is the series of field-strength contours.

Another matter which intrigued me very much, and on which I should like the authors to comment, concerns the wavy lines in Fig. 2(*a*) and particularly in Figs. 5(*a*) and 5(*b*). The field-strength record appears to be suffering from some type of vibration which modulates the record in a horizontal direction. I wonder whether this is due to a vibration of the pen recorder caused by the motion of the van, and, if so, why all the records do not show the same type of phenomenon.

Referring to the papers by Dr. Saxton and Messrs. Luscombe and Bazzard, I should like to inquire what exactly is the difference between a temperature-inversion layer and a duct. I know that Fig. 12 in the Part 2 paper shows an elevated temperature inversion plotted from meteorological radio soundings. That temperature inversion is, I think, some 50 m thick, and I am well aware that the authors have given the impression that a duct must be at least 150 m thick before it justifies the term "duct." On the other hand, I notice that Fig. 9 in the Part 1 paper considers duct widths going right down to zero. It is not quite clear to me, therefore, exactly at what stage a duct becomes a temperature-inversion layer.

Mr. E. Knighting: The first point I should like to consider is the scattering theory. It is very difficult to apply, largely because there is a parameter l which—as Dr. Saxton says—describes the scale of turbulence. It is commonly taken in radio work as varying between 10 cm and 10 m. Let me recall for you that the 10-m l was found by Scrase from some records made in the 1920's, using a Dines pressure-tube anemometer—a comparatively coarse instrument. The smaller values of l, about 10 cm, have been found by more recent workers using hot-wire anemometers. When one wants to find l, which is defined by means of an auto-correlation, one has to examine the continuous record that the recording pen has made on a sheet of paper. The value of l that one finds is a function of the lag and sensitivity of the instrument that was used. The Dines anemometer is not much affected by small eddies because the instrument has not sufficient time to acclimatize itself before the small eddy has gone and another one comes along. It reacts to fairly large eddies. The hot-wire anemometer, a much more sensitive instrument, reacts to quite small eddies so that auto-correlation on the record is lost more quickly; it is not surprising that one finds a much larger l in using the Dines instrument than in using a hot-wire anemometer.

The real answer is that all these l's as measured by different instruments are in the atmosphere, and one cannot select a single value of l as measured by one instrument and say that this is a representative length of the turbulence in the atmosphere. There is a sort of spectral distribution of l with instrument, although some values will predominate. I think that Dr. Saxton was quite right in believing that l in the upper atmosphere would not be of very much importance from his point of view.

I should also like to point out that the sea waves—the white horses which are seen when the wind is above about 15 m.p.h.—might provide just as good a set of scatterers as do turbulent discontinuities in the atmosphere. I understand that the possibility is being investigated at the moment.

What admirably sound meteorology Dr. Saxton has given us. I can confirm that a duct height of 50 m must be rare in this country and, if such a height did exist, I doubt whether it would extend any great distance laterally. Dr. Saxton suggests that the duct height at the end of a clear night would probably not exceed 50 m in this country. The biggest ducts formed in perfectly dry atmosphere would not exceed 10 m, and, as recent measurements show, in practice a duct is not formed at all. As the air cools at the earth's surface, dew forms and an inversion of humidity as well as one of temperature takes place. This inversion of humidity inhibits any duct formation.

Mr. A. H. Mumford: In Section 3 of the paper by Dr. Saxton and Messrs. Luscombe and Bazzard, the authors suggest rather diffidently that they believe it is perhaps more realistic to attempt to relate the quasi-peak field strength to the meteorological conditions, but their reasons are not very clear or convincing. Whilst a statistical analysis is probably best for these particular problems, and such methods have been used for many years in studying propagation over non-optical links, the analysis would have been even more valuable to the communication engineer if it had been in terms of the percentage time for which the signal strength—not the quasi field strength—had exceeded given values. It would be very helpful if the authors could include some such data in their reply. The separation into three groups of the observations on the various links corresponding to morning, afternoon and

* McPetrie, J. S., and Saxton, J. A.: "An Experimental Investigation of the Propagation of Radiation having Wavelengths of 2 and 3 Metres," *Journal I.E.E.*, 1940, 87, p. 146.

evening periods is interesting. It is not surprising that a comparison of the analysis shows a difference between the three periods, but is it correct to interpret this difference, as the authors do in Section 5.1 for the Moorside Edge–Teddington link, as a "diurnal variation of signal strength which, on average, seems to have a range of about 10 db on any given day." It certainly seems improbable that this figure of 10 db can be applied to any given day. It would be very interesting to know what more direct analysis of the observed diurnal changes would show. The method of analysis has unfortunately masked any seasonal changes which might be present in the extensive series of observations, which lasted over a period of some 18 months, and perhaps the authors can say whether any seasonal changes have been detected.

In the Conclusions it has been pointed out that, whilst abnormal field strengths at long range often do not depend greatly upon aerial height, particularly when elevated reflecting layers are involved, the "normal" field strength within, say, 100 km of the transmitting station does depend upon the terminal heights. From this, the authors deduce quite rightly in Section 7 that intolerable interference might frequently occur between two v.h.f. broadcasting stations unless they were separated by at least 350–400 km.

However, another important conclusion can be drawn. Since an improvement in the normal field of either station can be obtained by increasing the height of the appropriate transmitting aerial without correspondingly increasing the level of the abnormal field which causes interference in the service area of the other station, and since a similar increase in normal field cannot be effected by an increase of power without correspondingly increasing the interference, the selection of the most effective location and aerial height for the transmitting stations is of the greatest importance.

Dr. I. J. Shaw: In presenting the paper by Messrs. Kirke and Ross and himself, Mr. Rowden has explained how, from a field-strength curve such as that shown in Fig. 14, for example, it is possible to assign an effective height to the transmitting aerial for a particular receiving point. This is done by noting that the measured field-strength at the point corresponds to the theoretical value calculated on the assumption of some particular transmitting aerial height. In Fig. 14, for example, the measured value of the field strength at a point about 11 miles from the transmitter falls on the theoretical curve for a transmitting-aerial height of 500 ft, and 500 ft is then taken as the effective height of the transmitting aerial for this particular receiving point.

This procedure does not appear to be entirely sound because it neglects the effective height of the receiving aerial, which is, of course, equally important. Referring again to Fig. 14, the field strength at a distance of 18 miles from the transmitter is seen to be very high. According to the procedure used above, it would be assumed that the effective height of the transmitting aerial is of the order of 3 000 ft. In this particular case, however, it seems likely that the high field-strength is due to the combination of large values of the effective heights of both the receiving and the transmitting aerials. The receiving aerial is situated near the brow of a ridge, and the point of reflection of the reflected waves may well be somewhere on the plane extending from 12 to 15 miles from the transmitter. If this were so, the effective height of the transmitting aerial would be of the order of 900 ft, whilst that of the receiving aerial would be of the order of 250 ft. If this point is not taken into account, the field strength calculated for an increased height of the transmitting aerial will be seriously in error.

Also in connection with this question of effective height, I should like to know how the authors correct the measurements made with the receiving aerial at 20 ft to an assumed height at 30 ft. For the particular point mentioned above, for example, an increase of 10 ft in receiving-aerial height would make very little difference to the received field, since the aerial already has an effective height of some 250 ft. But at 20 miles or more from the transmitter, an increase of receiving aerial height from 20 to 30 ft may be expected to give a gain in field strength of about 3·5 db.

Have the authors any experimental results on the type of field found behind a hill or similar obstacle? With a transmitter and receiver separated by an obstacle, there are four possible ray paths from the one to the other. One path involves no ground reflection, two others have one ground reflection, and the fourth is reflected twice. Each of the four components suffers a loss of amplitude and a change of phase on being diffracted. Provided that the diffraction loss is not too great, it should be possible to find points behind the obstacle for which the phases of the components are such that a resultant is formed which is very much greater than that formed from the two component waves which exist in the absence of the obstacle.

Mr. J. P. Titheradge: The Home Office, with which I am associated, is well equipped to carry out v.h.f. surveys on a wide scale and to delve into the problem from every possible aspect, but, in common with a large number of other Government Departments, it is handicapped by restricted expenditure and staff, and it has insufficient support to undertake this work. I have in mind many other organizations also, such as the Ministry of Civil Aviation, the B.B.C. and the G.P.O., as well as the various commercial users, Municipal and Public Undertakings, not to mention the Service Departments. The resources of these bodies, collectively, are of inestimable scope and value. If properly co-ordinated and directed, they could provide the answers to all v.h.f. propagation problems from every aspect within a very few years.

Dr. Saxton and his colleagues will, I think, agree with me that they have a great deal of ground still to cover; and also the subject is one of vital national importance. To realize that this is so, we have only to think of communications with aircraft, the Police Forces, the Civil Defence and other essential services in time of war. We must ensure the utmost freedom from mutual inteference and from the chaos which is likely to result when these services are required to operate simultaneously, as in an emergency. Definite knowledge of the long-distance propagation of v.h.f. radio waves is also of considerable importance in deciding on the security measures to be imposed on services using these frequencies in time of war.

I sincerely hope that this is realized in the highest circles. There are vast v.h.f. projects in the course of preparation in the United Kingdom, but no large-scale test of the simultaneous operation of these services has yet been made.

I do feel, therefore, that we must press forward with a thorough well-directed and co-ordinated investigation of all aspects of v.h.f. radio-wave propagation; in addition, the other factors which must be considered include the questions of man-made and static interference related to frequency, and the ranges possible between land mobiles, aircraft and ground, and other services.

Dr. E. C. S. Megaw: The complexity of the actual distribution of refractive index in the lower atmosphere has become increasingly clear as the application of the shorter radio waves has proceeded; the trend towards statistical methods of presentation and analysis has been a natural consequence. The application and practical utility of several different idealizations of this actual complexity has been well illustrated, especially in the clear discussion of partial reflection from elevated regions where the lapse rate of refractive index is large. It is perhaps desirable to emphasize the dangers of identifying the various theoretical idealizations too closely with distinct physical processes.

This is no less true of the recent concept of turbulent scattering than it is of the older ones like equivalent earth radius, radio ducts, and reflection from elevated layers, all of which have, on occasion, been the subject of dogmatic over-simplification. The success of the scattering concept in accounting for some unexpected features of the propagation of centimetre waves, in particular, and in relating them to the effects of atmospheric turbulence on visible light, has been encouraging; but I do not regard either the theoretical discussion by Booker and Gordon or my own as providing more than a starting point in this interesting field. Although the conclusions which Dr. Saxton and his colleagues have reached regarding scattering at metre wavelengths are broadly consistent with the tentative views I have recorded elsewhere,* I think that, for these wavelengths in particular, numerical relationships between radio results and measures of turbulence should still be treated with considerable reserve.

Discussion of atmospheric turbulence is likely to figure increasingly in radio-wave-propagation literature, and it is important that introductory statements should avoid adding to the real difficulties of an unfamiliar subject. In essence, turbulence is the irregular, eddying component of motion acquired by any stream of fluid unless it is very small, very slow-moving, or very viscous. Inspection of, say, a plume of cigarette smoke at one end of the scale, and a bank of clouds at the other, shows that the range of "eddy sizes" in the atmosphere—which can be discussed precisely in terms of an average spectrum of Fourier components—is very large. This statistical eddy-spectrum, which is related to the spectrum of refractive-index fluctuations, can be specified in a number of different ways, either directly or in terms of a Fourier transform defining the correlation between fluctuations at different points in space. Although the range of atmospheric eddy sizes is large, it cannot be infinitely large, as is tacitly assumed in the formulation used by Dr. Saxton; to specify a physically realizable spectrum, at least two numbers need measuring, for instance, the sizes of the largest and smallest eddies. The quantity misleadingly called *the* scale of turbulence in the paper is, in practice, a measure of the size of the *large* eddies which contain most of the kinetic energy of the turbulence. It should be called the *integral* scale—because it is defined as the infinite integral of a correlation function—to distinguish it from the *micro*-scale which is a measure of the distance over which fluctuations are strongly correlated. The micro-scale corresponds rather loosely to the size of the *smallest* eddies, though it does not actually define this size. This is by no means the end of the variety of scales which have been defined in the literature of turbulence; but since the integral scale has already been confused with the much smaller micro-scale in a discussion of the radio problem elsewhere, this distinction is one which should be emphasized at the outset.

Capt. R. Danzinger: It is known that propagation of v.h.f. radio waves along coastlines is rather anomalous, and I should like to know whether any experiments were made under these circumstances. I would also ask, still referring to propagation in coastal areas, whether there exists a propagation cycle, which repeats itself over a 24-h period or, in other words, whether optimum conditions exist at the same hour every day, say in the afternoon, when the subsidence decreases. Do such conditions occur at the same time of the 24-h cycle all over the world—e.g. in Florida and in California?

Whilst speaking of v.h.f. radio-wave propagation beyond the normal horizon, I should like to mention that, in the summer of 1948, the television audio signal was received very well on frequent occasions in Israel.